

AN EXAMINATION OF THE CLASSIFICATORY AND SOME OTHER RESULTS OF EIMER'S RESEARCHES ON EASTERN PAPILIOS.

A REVIEW AND REPLY.

By KARL JORDAN, PH.D.

THE following lines were written before the death of Professor Eimer. On receiving the sad news of the untimely departure of the ardent defender of the inheritance of acquired characters, I have altered the review in all those points which relate to debatable matter ; but I could not suppress it altogether, as Mr. Rothschild and I had to give a reply to Professor Eimer's sharp-worded remarks in his last book (*Orthogenesis*), and as, further, we thought it our duty to show how far the facts brought forward by Eimer as a basis for general deductions harmonise with the results of our studies on Papilios.

In the introduction to Mr. Rothschild's Revision of the Eastern Papilios (Nov. Zool. 1895) I gave a brief survey of the principal works dealing with the Papilios of those regions, and had occasion to allude to Eimer's book on *Artbildung und Verwandtschaft bei den Schmetterlingen*, stating (*l.c.* p. 174) that Eimer's and Fickert's papers were "of little consequence for the systematic worker"—meaning the classifier—"as both authors apparently employed too small a material to enable them to avoid grave errors in respect to the relationship of the various Papilios." In his recent book *Orthogenesis der Schmetterlinge*, p. 45, Eimer complains of that remark, and maintains that the criticisms by Mr. Rothschild* were partly erroneous, and partly advanced without a sufficient support by facts being given. If Eimer were right in repudiating the corrections, I should certainly withdraw the above remark without hesitation. Unfortunately, a study of Eimer's second and third book† and renewed examination of the Papilios convince me, not only that Mr. Rothschild's and my criticisms were well founded, but also that the errors in classification were much less due to insufficient material than to oversights in his one-sided researches. A conviction, however, is of little value ; facts and arguments must be brought forward. As Eimer protested that he was right in those cases in which Mr. Rothschild and I said he was wrong, it is necessary for me to show—in order to avoid the reproach of unfounded criticism—that my remarks, both the one in Nov. Zool. 1895. p. 174 and the one *ibid.* 1896. p. 507, are wholly justified by facts.

The object of Eimer's works on Lepidoptera was twofold : the researches on Papilios were undertaken (1) to demonstrate the phyletic connection between certain Papilio forms by means of a comparison of their wing-pattern (*Verwandtschaft*), and

* I did not think it necessary to give a separate answer to the remarks in *Orthogenesis*, pp. 44-46. As Dr. Jordan had necessarily to refer to the disputed points as well, my reply is embodied in the above review.—W. R.

† In order to avoid unnecessary repetition of the titles of the three books, I shall cite the works as *Artbildung* I., II., and *Orthogenesis*.

Artbildung und Verwandtschaft bei den Schmetterlingen I. 1889.

" " " " II. 1895.
Orthogenesis der Schmetterlinge. 1896.

(2) to find affirmative evidence for certain conclusions of a general nature relating to the origin of species (*Arbildung*).

As the study of the Papilios was a means to a certain end, it might justly be claimed that mistakes in the descriptive and classificatory parts of Eimer's works, or the errors *re facts*, are of importance only if they have a bearing upon the general inferences. For that reason we will let pass as unimportant the omission of a number of interesting varieties (*P. alebion* ab. *mariesi*, *P. antiphates pompilius* ab. *nebulosus*, *P. antiphates antiphates*, *P. nomius swinhoei*, *P. paron*, *P. machaon asiaticus* ab. *ladakensis*, etc.), though in a special paper devoted to the classification and particularly to the variation of a small number of species one would expect to find mentioned at least all the varieties which have been known to science for a longer time. Nor will I lay any stress upon the introduction of new names for forms which have already names—the N.W. Indian *machaon* is described as a new subspecies *punjabensis*, though it has already two names, *asiaticus* and *ladakensis*; nor upon erroneous denominations—Eimer's *P. machaon asiatica* is really *P. machaon sikkimensis*. And I will also consider it of no great moment that the *habitat* (1) of *P. aristus hermoerates* is given as "Philippinen (Manilla)," while the insect is known to have a much wider distribution, (2) of *aristus* is said to be "Molukken (Batjan)," though the insect was first described from Amboina and has long been known from the Southern and Northern Moluccas, (3) of *P. xuthus* is recorded as being "Ostasien" and that of "*xuthus* var. *xuthulus*" as being "Ostsibirien," while in fact the one is the summer, the other the spring form of a species that in 1895 was already known to occur in Japan, Amurland, and China; and I will only cursorily mention that neither *P. eurymedon* is confined to California, nor *daunus* to Mexico, Texas, and Kansas, both having a much wider range, and that it is a geographical enigma, if it is said that *P. antiphates* ranges from the "North Indian Islands" into Asia, considering that this Papilio is found from Ceylon, Sikkim, Southern China, Burma, Malacca, the Andaman Islands, to Flores, the Philippines, and the Northern Moluccas. Such small matters, though in themselves of no great weight, are nevertheless of significance, inasmuch as they show that the literature on the insects dealt with was not extensively studied. I know it is a heavy task toiling through the mass of literature relating to Papilios, and I am quite ready to excuse such slips as those mentioned here. But if some one of my readers would seek information about the Papilios treated upon in *Arbildung*, I must recommend him the books and treatises of entomologists, where he will meet with the record of many important facts which are not to be found in *Arbildung*. For instance, he will learn the fact, well known in entomology, that Palaearctic forms of Lepidoptera occur in the tropics generally at high elevations; and as he must conclude that this is probably also the case with *P. machaon* in India, he will consequently have some doubt about the correctness of the locality of Eimer's Allahabad *machaon*. Literature will further tell him that a species varies geographically, and he will hence expect that, if *machaon* really occurs in the hot valley of the Ganges, this *machaon* were very different from the mountain form of N.W. India; and as there is no such difference, the student of the literature will rightly conclude that the said Allahabad specimen is not from there. Lepidopterists know very well that Japan has not that large *machaon* form *hippocrates* only, alone known to Eimer, but that there is a spring brood of this large summer brood which is much more similar to the European *machaon* than the latter; the student of the literature will not miss the very conspicuous and very peculiar phenomenon that the differences in the development of black of the upperside between the summer and spring

broods in Japan are the reverse of the differences in colour between the summer and spring broods of *machaon* in Europe ; he will see that in Japan the black colour has considerably increased in the summer specimens, while in Europe the black colour has decreased in the summer brood—a phenomenon which will make it evident at once (1) that, if heat is (as maintained by Eimer and others) **the** factor which produces the characters of the summer broods, the action of the same factor has opposite results in these forms of *machaon*, which means that difference in colour is not a good measure of relationship, or (2) that the factor or factors which produce the characters of the summer brood in Japan are different from the factors to which in Europe the summer broods are due, which means that we do **not** know whether it is heat here and some other factor in Japan, whether the reverse is true, or whether heat is at all a *prima causa* of the characters of the summer broods. Works on Butterflies will tell the student that transitional specimens between the ordinary and the black female of *P. turnus** are recorded and are figured in Edwards, *Butterflies of North America*, 2nd series, *Papilio* t. V. (1884), though it is said again and again in *Artbildung* that there are no such transitions, the absence of such transitional individuals being again and again given as the main argument for the origin of species *per saltum*. Such and similar facts of importance we learn by looking over the writings of entomologists. If the author of *Artbildung* had taken the trouble to find out what was known to science about the *Papilios* which he selected for his researches, he would not have considered in 1895 *P. xuthus* to be subspecifically distinct from *xuthulus*, as the summer form *xuthus* had already in 1875 been bred from the spring form *xuthulus*, and as this has since repeatedly been done. The case of *xuthus* and *xuthulus* is, however, of significance in another respect. If one knows that the small and the large, aberrant-looking, Japanese forms of *machaon* stand in the relation of spring and summer broods to each other, the suggestion that *xuthulus* and *xuthus*, which differ in a similar way, stand in the same relation must present itself. If one has a material sufficiently large for the study of variation, one must see that *xuthus* and *xuthulus* cannot be specifically distinct, as there are all intergradations. If one compares the sexual organs of the very different specimens of *xuthus* and *xuthulus*, such as were alone known to Eimer, one must become at least doubtful that one has to do with different subspecies or different species, as the sexual organs do not show a distinguishing character, as species and marked subspecies do in the case of *Papilios* with a more complicated structure of the *male* genital armature. In *Artbildung* only the wing-pattern of *xuthus* is taken into consideration, and this did obviously **not** show the author the true relation between *xuthus* and *xuthulus*. The entomologist who knows that authors have so often been deceived by the flexible wing-pattern, and who has the case of *xuthus* and *xuthulus* before him, will *a priori* not be inclined to accept without further inquiry classificatory results which are based only on the comparison of wing-patterns, though he reads in *Artbildung* I. p. 23, “The main point, however, is, that by my researches the principal traits of the real relationship of the forms are determined, and that thereby a handle is given to make the ‘system’ in our branch of zoology what it really should be, the expression of blood-relationship”; and in II. p. 66: “Whoever will dispute after the appearance of my work, that the pattern is the most essential and the most infallible guide for the recognition of the relationship of the Butterflies and for the understanding of the laws that govern the origin of species, must necessarily have

* The oldest name is *glauca*, but I use throughout this rejoinder the name of *turnus*, in order to avoid confusion.

approached the subject with the same delight in 'opposition' and contradiction as Erich Haase." Would it not be better to say that he who does not accept without critical examination what is told him is the true scientist? But let us now examine the classificatory results of the researches laid down in *Artbildung*.

Eimer says that his researches reveal the phyletic connection between the forms treated upon; hence we must accept the "groups" of species formed by him to contain only such species which are more nearly related with one another than with the members of other groups, except if one group has directly developed from a species of another group. If we bear this in mind, the enigma embodied in the *leosthenes-antocrates-ajax* (*Artbildung* I. p. 156) and the *ajax-policeues* groups (*ibid.* p. 193), and in the *turnus* (*Artbildung* II. p. 79) and the *asterias* groups (*ibid.* p. 118), is insolvable, and we must ask what profound mystery is at the bottom of the association of *P. ajax* and *turnus* each with **two** groups of species. Is the black female (*glaucus*) of *turnus* more nearly related with *P. asterias* than with its own male? Is *walshi*, though a seasonal form of *ajax*, less closely connected with its sister forms, which partly originate every year from the eggs of the same mother individual, of which also *walshi* is an offspring, than with the African *P. policeues*? The offspring of one female belong to two different groups of species?

In his first volume Eimer deals with four "groups" of Papilios, which all belong to that section of *Papilioninae* to which Haase gave the name *Cosmodesmus*. Group characters, that is distinguishing characters of each group, are not given; in fact there are no such characters common to the members of one and absent from the members of the other groups, with the exception of the *antiphates* group, which contains only the geographical representatives of *antiphates* and some close allies. But it is obvious that the reason why out of the great number of Asiatic, African, and American species of *Cosmodesmus* just those particular forms were united into groups regardless of all characters except wing-pattern, was that the wing-pattern of the forms put together in each group demonstrated a connection between the species in accordance with Eimer's views. Species, however, put together and treated as relatives **because** their wing-patterns exhibit certain (real or supposed) affinities, cannot be brought forward as demonstrating that wing-pattern exhibits the phyletic connection admirably, and shows the lines of development by which each species, by which each character, has arrived at its present state in mutation. If one intends to demonstrate the kind of variation of a particular organ or character by a comparison of this particular organ or character in closely allied species, it is absolutely necessary to ascertain that the species to be compared are related to each other, not because they are similar in that particular organ, but because other characters, which are independent of that organ or character in their variation, establish the relationship. Eimer rejects other characters than wing-pattern as being of inferior value. Haase* pointed out that Eimer's classification was faulty, because structural characters, especially a very striking one in neururation, had been neglected. The first subcostal branch is, namely, in a number of species of Eimer's groups I., II., III., and IV. invariably anastomosed to the costal nervure (for instance, in *glycerion*, *paphus*, *agetes*, *antiphates*, *aristeus*, *rhesus*, etc.), while in other species of these groups that vein is free (for instance, in *podalirius*, *leosthenes*, *ajax*, *protesilaus*, etc.). The development of the neururation in the pupal wings shows conclusively that all nervules were originally free, and that fusion and obliteration are specialisations. Hence the species with the first subcostal branch free are in this

* Haase, *Untersuchungen über Mimicry* I. 1893.

respect more generalised than the species in which that vein joins the costal nervure. Eimer has, therefore, united in his groups I., III., and IV. species which according to the venation do not belong together. However, the argument from venation is in *Artbildung* II. p. 60 altogether rejected by Dr. Fickert. As the agreement in the specialisation of the venation was one of the reasons which induced Mr. Rothschild to bring the Indo-Malayan *agetes* in its proper place near the Indo-Malayan *antiphates* and Bornean *stratiotes* instead of near *podalirius*, the Celebensian *rhesus* near the Indo-Australian *aristeus*, etc., let us examine the objections Dr. Fickert has to make. Dr. Fickert states that (1) the first subcostal branch joins the costal nervure in *P. alebion*, *glycerion*, *agetes*, *rhesus*, etc., etc., while it is free in *podalirius*, *leosthenes*, etc.; (2) the point of origin and the length of this branch are variable; (3) the first subcostal branch is wanting altogether in *P. bellerophon*; and then proceeds to say that the absence of the first subcostal branch in *bellerophon* "**would, if it had been recognised**, probably alone have sufficed, under the reign of the doctrine of the wing-venation of Lepidoptera, for the erection, if not of a special genus, at least of a subgenus for *P. bellerophon*. If one has, however, once closely examined a larger number of individuals of one species, or species of one genus, with regard to the venation, and has thereby found that a greater number of veins, especially the costal and the first branches of the subcostal veins, vary more or less in their characters, one will no longer lay too great stress upon such small differences. It is, therefore, my opinion that there is no reason at all to separate *P. agetes*, which by the way also C. and R. Felder consider similar in appearance to the *protesilaus* group, from this group, and also to separate *P. leosthenes* on account of the different course of the first subcostal branch from the otherwise so closely allied *antierates*," etc.—Dr. Fickert misses altogether the point at issue: for it is not the length of the first subcostal branch, nor its point of origin, that is maintained by Haase, Rothschild, and others to be of classificatory significance; no, the important point is that the first subcostal branch is **invariably fused** with the costal nervure in *alebion*, *antierates*, *agetes*, etc., and **invariably free** in *podalirius*, *leosthenes*, *protesilaus*, etc., a fact which Dr. Fickert has verified. A variable character is substituted for a constant one; by showing the first to be of no classificatory value, the importance of the second is surely not lessened to the slightest extent. The reader will now be able to judge for himself whether there is a sound basis for the following sentences in *Artbildung*: "The venation stands in the background, as compared with pattern, in regard to the establishment of the relationship, and only certain veins are here of importance; others, and just those which Mr. Haase relies upon in his opposition against me, are so liable to variation that they can by no means be made use of. Here belong especially the veins of the anterior margin of the forewing" (II. p. 59). "That the venation of the wing must also be of weight in establishing the relationship of Lepidoptera is self-evident. But it is, according to what is said before, a great mistake to assume that every venation indiscriminately must be of importance. I have not taken venation into consideration either in the case of the Swallow-tails or now in the case of the 'Segelfalter,' **because we have not found anything that contradicted the relations** [black type mine] which are maintained by us and are so clearly demonstrated by the pattern." It sounds rather queer that the subcostal veins cannot be of any taxonomic value, because they are variable, if we bear in mind that the wing-pattern, the basis of Eimer's classification, is extremely variable. Whether the absence of the first subcostal branch from *bellerophon* will or will not justify a generic separation

of this species from the other *Cosmodesmus*, I will not enter upon; but I must mention that the absence of a subcostal branch was one of the chief characters that induced Salvin—who knew the *Papilio*s of America very well, and not only their colour, but also their structure—to erect the genus *Baronia* for a Mexican species, *baroni*.

The specialisation in neururation is not the only character in which the Indo-Malayan *P. agetes* differs widely from the Palaearctic *podalirius* and the American *protesilaus* and *bellerophon*, and in which it agrees with *antiphates*, *stratiotes*, *aristeus*, and other species of the Indo-Australian fauna; but exactly the same close connection is demonstrated by the genital armature, which is at a glance recognisable as being built up after the same type as in *agetes*, *antiphates*, etc., and after entirely different types in *podalirius*, *protesilaus*, etc. While *P. agetes* stands in group I., the North Bornean *P. stratiotes* is placed near *antiphates* in group II.; both insects agree, however, so closely with one another, besides in neururation, in the structure of the genital armature and the presence of a large cottony patch in the abdominal fold of the *males*, and are also in pattern so similar to each other, that there is no justification whatever for linking *agetes* to an aberrant American species (*bellerophon*) and separating it altogether from its very close relative *stratiotes*. A most remarkable character in pattern common to the two insects is found by Eimer himself; that is the presence of a large band in the same place where in the other species band vii. stands, a band which should really be absent from *agetes* and *stratiotes* according to Eimer's "laws" of development. In *Orthogenesis*, p. 45, it is said that Mr. Rothschild's collections have not brought forward any intermediate forms between *agetes*, *stratiotes*, and *antiphates*. A complete series of intergradations certainly cannot be expected, because the three insects are three very distinct species; but Mr. Rothschild has shown (Nov. Zool. II. p. 417) that *agetes* is in pattern to a certain extent connected with *stratiotes* by the Malayan subspecies *P. agetes insularis*, and that *stratiotes* does stand intermediate in pattern between *agetes* and *antiphates*. It is rather surprising to read (*i.e.*), *re* the position of *P. agetes*, that Eimer finds only **general assertions** brought forward by Mr. Rothschild instead of facts, if one knows that Mr. Rothschild gave as his reason for placing *stratiotes* and *agetes* together that they agree in neururation, in the *male* scent-organ, and in pattern.

A second correction made by Mr. Rothschild relating to a species of Eimer's group I. is also rejected by Eimer. In *Arbildung* I. p. 65 we are told, under *P. alebion*, that "Oberthür, from the comparison of the figure of Gray, will erect a new species, which he called *P. tamerlanus*, on the ground of [differences in] colour and the general aspect (!). This *tamerlanus* is simply an *alebion*." In Nov. Zool. II. p. 409 it was shown that Eimer made a mistake in identification, Oberthür's *tamerlanus* being different from *alebion* in the pattern and the shape of the hindwing especially; and Eimer's *alebion* being this *tamerlanus*, not Gray's *alebion*. An error in identification is of no particular weight, and I should not have mentioned the mistake here, if the reply in *Orthogenesis*, p. 45, were not so significant: "That my *alebion* be not this, but *tamerlanus* Oberth., is settled by the fact that one of the best informed students of exotic Lepidoptera, **Staudinger**, does not regard *tamerlanus* as specifically distinct, but considers it to be synonymous with *alebion*; it (*tamerlanus*) can at the highest certainly only be an 'Abart,' for the separation is founded in Rothschild's *tamerlanus* on nothing else but the division of the yellow anal spot into two spots, a division which occurs also in seasonal varieties of *P. ajax*, namely in *walshi* and *tetamonides*—in the latter the spot is sometimes divided sometimes not!"—My answer is: (1) that Dr. Staudinger probably did not know

alebion, but only *tamerlanus*: his mistake **explains** Eimer's mistake, but is surely not an argument that Eimer was **right** in his criticism of Oberthür, who described and figured (!) *tamerlanus*; besides, in a treatise on a special subject, the author should judge for himself; (2) that, besides the difference in the anal spot, Mr. Rothschild noticed also a marked difference in the shape of the hindwing; (3) that the circumstance of non-specific differences being found between the forms of widely different *P. ajax* which are quantitatively similar to the differences in pattern between *alebion* and *tamerlanus* cannot possibly be advanced by the author of *Arbildung*, as it is one of Eimer's main contentions that characters which in one case are merely individual are in other cases subspecific and specific—an opinion which nobody will contest, if very distantly related forms only are taken into consideration. It is scarcely necessary to mention that the habitat "Nord China" given by Eimer for his *alebion* = *tamerlanus*, not Gray's, is incorrect: his specimen was doubtless from Western China, where *tamerlanus* is not a rarity: "North China" was given as the habitat of the true *alebion* by Gray.

Eimer's third group contains again a mixture of Indo-Australian and American forms. Here we find *P. leosthenes* brought in close connection with *P. aristeus*. Haase's and Rothschild's contention was that *leosthenes* is closer related to *podalirius* than to *aristeus*, a contention which I consider perfectly correct. *P. leosthenes* is not a **very** near ally of *podalirius*, but, as one has to place it somewhere, it will find its place best near that species. For *P. leosthenes* agrees with *podalirius*, and disagrees with *aristeus*, in neuration, the first subcostal nervule of the forewing being free; besides, the morphological characters of the end of its abdomen are not in accordance with those of *aristeus*, being, as in *podalirius*, of a less specialised type; and the wing-pattern is also certainly not against a classificatory connection of *leosthenes* with *podalirius*. For what is said about the pattern of *leosthenes* in *Arbildung* I. pp. 158, 159? (1) In the two marginal bands *leosthenes* resembles *podalirius*; (2) the pattern of the upperside of the hindwing corresponds almost entirely to that of *podalirius*; (3) also the pattern of the underside of the hindwing is essentially the same as in *podalirius*; (4) the interspace between the two marginal bands of the forewing is as in *podalirius*, not as in *aristeus*.—There is nothing said about similarities between *leosthenes* and the species with which it is placed together, nor is any reason given why those similarities between *leosthenes* and *podalirius* are disregarded in the classification. It is obvious that, by thus placing species like *podalirius* and *leosthenes*, in spite of their agreement in pattern, into different groups, it is easy to demonstrate the appearance of the same characters in members of different groups, a fact which is much more likely to discredit "Homoeogenesis" than to confirm it. Incidentally I may mention that Eimer says of *leosthenes* that band vii. has entirely disappeared, while it is, in fact, indicated by a spot in about 75 per cent. of the specimens examined by me (it was my contention, in Nov. Zool. II. 1895. p. 174, that Eimer had worked with too small a material).

But there is one other objection advanced against the connection of *podalirius* with *leosthenes* in the reply to E. Haase's statement, that in *Arbildung* I. the geographical distribution had often been left out of consideration. Haase, in opposition to Eimer, considered, like Felder, geographical distribution one of the most important arguments for the establishment of natural groups of species. As this argument from geographical distribution relates not only to *leosthenes*, but also to other species I shall have to treat upon later on, we will examine this question here once for all. In order to reject Haase's criticism, Dr. Fickert first reproduces

(II. p. 56) Sections XIX. to XXVI. of Felder's classification of Papilios, meaning to show that geographical distribution did **not** always find an expression in Felder's classification. For, he says, though the species in the sections are put together geographically, the species are nevertheless partly from very distant areas, for instance in Section XXI. This section contains only species inhabiting the countries from China to Australia. Does Dr. Fickert believe that there is something wrong, zoogeographically, in uniting Chinese, Indian, Malayan, and Papuan forms in one section? I think he does; for Eimer is of the same opinion. We read in II. p. 63: "Herr Haase will establish, on the ground of the nomenclature, relations between the *alebion-glyceion-paphus* group with *agetes-antiphates-anticrates* and also with the African *policeus-antheus*. That is meant to be natural geographical grouping! Besides, we must say that it is quite impossible to bring the North Indian and North Chinese Butterflies, like *alebion-glyceion-paphus*, in geographical connection with those from South India and the Malayan Archipelago (*antiphates, antierates*). The North Indian and North Chinese fauna joins, on the contrary, towards west the European one, as is also demonstrated by *machaon*. However, that the same author who continually boasts of having regard to the geographical connection will even bring the Indo-Malays in relation with the Africans is surely very strange. . . . Downright astonishing geography . . . it is, if the Australian (!) *leosthenes* is joined to the European *podalirius*."

I do not believe that any of the readers of *Arbildung und Verwandtschaft* are so in ignorance of the most simple facts of the science of geographical distribution, that they do **not** know that the greater proportion of Chinese—*alebion* is a Central Chinese, not a North Chinese, and *tamerlanus* a West Chinese species—and North Indian species extend into the Malayan or even Papuan sub-regions; that the South Indian fauna consists for the most part of modified North Indian species; that the fauna of Queensland, where *leosthenes* lives, has very close affinities to the Indo-Malayan fauna; and that tropical Africa, especially the forest-clad West African countries, stands zoogeographically in close connection with India.

But if it were so "downright astonishing" to unite a Palaearctic species that extends into China (*podalirius*) with an Australian species (*leosthenes*) into one group of species, would it not be much more wonderful to have one and the same **species**—as Eimer maintains—in North India and Queensland, while the intermediate countries are inhabited by other representatives? Though it was pointed out in Nov. Zool. II. p. 419 that the North Indian *antierates* and the North Australian and Papuan *parmutus* are not identical, as asserted in *Arbildung* I., the correction, besides others relating to the forms of *aristeus*, is altogether rejected in *Orthogenesis*. We read there as follows (p. 45):—

(1) "As regards the correction in respect to my *P. aristoides, antierates nigricans*, and *aristeus nigricans*, namely that they all are *hermocrates*, it is sufficient to point out that the original determination of my *aristeoides* as *nomius* var., of *antierates nigricans* as *antierates* var., and of *aristeus nigricans* as *aristeus* var. proceeded all from Dr. Staudinger and that the respective types belong to Staudinger's collection, so that Mr. Rothschild, if he will prove mistakes, must apply to Mr. Staudinger."—Dr. Staudinger, who is known to lend material with the greatest liberality to students, is not responsible for the contents of papers written with the help of his material; Eimer has baptised those forms, not Staudinger. The types of Eimer's *aristeoides* and *antierates nigricans* came both from Upper Burma,

and there is nothing whatever in the pattern of this *nigricans* (according to Eimer's figure) that speaks against its being a somewhat melanistic individual of *aristeoides*, instead of a specimen of the North Indian representative *antierates*.*

(2) "Further, I must say that Mr. Rothschild should have given some proof, or at least some reason, for the union of *hermocrates*, *aristeus*, *antierates*, and *parmatius* to one species, and their denomination as 'local races.'—It has been stated in Nov. Zool. II. p. 179 what forms are considered "local races." The forms here mentioned are geographical developments of the same species, inhabiting separate but continuous areas, and their characters in pattern are such that there is no marked line of distinction between them, as **was** pointed out by Mr. Rothschild, *l.c.* p. 421. I may add that *aristeus antierates* and *aristeus hermocrates* are, in the structure of the genital armature, perfectly connected by *aristeus aristeoides* (see Nov. Zool. III. 1896. p. 487).

(3) "In any case, Mr. Rothschild takes the term 'local race' in a very wide sense, since *hermocrates* lives on the Philippines, *aristeus* on the Moluccas, *antierates* in North India, and *parmatius* in North India and Australia!"—The sign of exclamation shows that Eimer means to say that the separate areas of the forms are too far distant from one another to admit of the insects being local races of one species. I reply that the geographical distribution of the forms is very inaccurately stated: for *antierates* occurs in North India, the lower coast regions of Tenasserim, Malacca, and Sumatra†; *aristeoides* occurs in Upper Burma (it is probably the Indo-Chinese form); then follows *hermocrates* from Borneo to the Philippines and southward over Kalao to Timor; farther east we find *aristeus* on the Southern and Northern Moluccas, and *parmatius* in New Guinea, Waigen, Aru, and Queensland. There are only two gaps in the distribution; the one is Java, where no representative of *aristeus* is found, and the other is Celebes, where a close ally of *aristeus*, namely *rhesus*, lives, which Eimer considers to be an immigrant from America. The facts that Java and the Andamans have no *aristeus* and that the Sumatra individuals are not distinguishable, so far as I see, from North Indian individuals, further that the specimens of *hermocrates* from the lesser Sunda Islands are on the whole indistinguishable from those from the Philippines and Borneo, suggest that *aristeus* is an eastern species that has spread westwards over the Moluccas, Celebes, the Philippines, Borneo, to India, and that a more recent migration in a southern direction has taken place. Besides the inaccuracies in the geographical distributions of the forms, Eimer's reply contains again an error in classification concerning *parmatius* and *antierates* which was already corrected in Nov. Zool. II. 1895. p. 419. The facts are these: *antierates* was described from Assam, it is known also from Sikkim, the Mergui Archipelago, Penang, and Deli (Sumatra); *parmatius* was described from Queensland, but is known to occur also in New Guinea, Waigen, and Aru. The differences between the two forms, which in "general aspect" are similar to each other, are such that in structure *parmatius* agrees with its geographical neighbour *aristeus*, while *antierates* is constantly different (see Nov. Zool. III. 1896. p. 487); in colour all the Indian specimens are distinguished from all the

* The melanistic specimen called *aristeus nigricans* I have examined lately, and find that it really is an individual of *aristeus*; its name should be *aristeus* ab. *nigricans*. I was misled by Eimer's description of the underside, which is said to be "golden-brown," while the underside of *aristeus* was described as black. The individual *nigricans* has the underside, however, only a little paler than *aristeus*, and this is probably due to the specimen being a rather old one.—W. R.

† I thought in 1895 that Sumatran specimens, which I had not seen, were *hermocrates*; I now know that they are *antierates*.—W. R.

Australian and Papuan ones by the two markedly yellow spots of pronotum of *parmatius* being obsolete, in the white costal bands of the forewing being much less straight, somewhat irregularly curved, especially in Sikkimese individuals, further in the black marginal area of the hindwing, above and below, being narrower behind and its inner edge indented upon veins 5 to 8, and in the underside being much paler brown. We have, therefore, to do with two forms easily distinguishable from one another also in pattern. What is made of them in *Artbildung* I.? *P. aristus* *anticrates* is described and figured from Sikkim specimens; further, it is stated that Gray based his *parmatius* on specimens in which band ix. (in Eimer's sense) of the forewing does not reach the hindmargin of the wing, and an Australian and a Sikkim individual are figured as *parmatius*. The figure of this Sikkim "*parmatius*" has, however, as a matter of course, on the wing the characters of *anticrates* and not those of *parmatius*, and shows them very obviously (compare *Artbildung* I. t. 3. f. 6. 7. 8). The difference upon which Eimer relies, namely the shorter band ix. in *parmatius*, does not hold good: this character occurs both in Indian and Australian specimens, and is neither here nor there constant: the real distinguishing characters between the Indian and the Australian forms Eimer has not seen, and that is the reason why he mixed the two forms up. It would be a simple oversight, and of no great weight in the judgment of the classificatory results of *Artbildung*, if the wide geographical separation of the Indian and Australian specimens should not have made the author very suspicious and careful. No doubt, superficially *parmatius* and *anticrates* are much more similar to each other than to *aristus* and *hermocrates*, which inhabit interjacent countries, on account of the great development of white in the first two; but if one compares them minutely with the object of demonstrating laws of development, one must soon see that also in pattern the Papuan *parmatius* stands closer to the Moluccan *aristus* than to the Indian *anticrates*. It was perfectly correct to treat *anticrates* and *parmatius* as two separate geographical races, as has been done in Nov. Zool. II. p. 419, while it is wrong to unite them in the way as in *Artbildung* I. p. 156, where we find:—

anticrates Doubl. } *anticrates* mihi.
parmatius Gray }

(4) "Though Mr. Rothschild unites thus"—namely *anticrates*, *hermocrates*, *aristus*, and *parmatius* as local races of one species, see above under (3)—"it is in his eyes a mistake that I regard *parmatius* as an 'Abart' of *anticrates* and not as a local race, as he does! These are surely strange criticisms, which are perfectly on the same level with those of Erich Haase."—I have not translated the word "Abart," because the usual translation "subspecies" has an entirely different meaning, being nowadays restricted as a term for local races. Eimer's "Abart" is here, however, the same that he in other places correctly calls individual aberration = "Abartung," Eimer's "Abart" *parmatius* (not Gray's) comprising such individuals of *anticrates* which have a certain individual distinguishing character, namely a shorter band ix. than the other individuals from the same place. We know that there is a wide distinction between such individual aberrations and local forms, and it was certainly wrong to consider slightly aberrational Indian specimens as identical with a well-marked Papuan subspecies. That the criticisms in Nov. Zool. are said to be on the same level with those of Haase is very acceptable, in so far as Haase was perfectly right in the two main points of his criticisms, respecting geographical distribution and nervation.

In the fourth group (*Artbildung* I. p. 192) Eimer unites species from

"America, West and East Africa, Madagascar, and India," nine altogether, and brings in his first subgroup the North American *ajax*, the Central American *philolaus*, and the Celebensian *rhesus*. If one selects out of some hundreds of forms of *Cosmodesmus* just these three as being most closely allied with one another, there must surely be some very strong evidence for the correctness of this selection, as it is *a priori* highly improbable that a Celebensian *Papilio* should have its nearest relative in the Nearetic fauna. It has been noticed in *Artbildung* I. that the association of *rhesus* with *ajax* and the African *colonna* and *antheus* looks strange, but we are told (*l.c.* p. 194) that "the **certainty** of the derivation of all is the more surprising." The *rhesus* question throws so much light upon the kind of treatment of the *Papilios* in *Artbildung*, that I hope to be excused to refer to Eimer's evidence and arguments more extensively:—

(1) "The explanation of the relationship of *rhesus* meets with difficulties from geographical arguments. . . . There are no species in its country, namely in Celebes, with which *rhesus* could be brought in immediate connection. The only possibility would be, that it had originated from a form similar to *leosthenes*, *hermocrates*, *nomius*, or *aristeus*, or a form which was much more ancestral than these. . . . One must keep in view the possibility that, in spite of the great distance between America and East India, eggs, larvae, or imagines of *ajax* or species similar to *ajax* had been transplanted to Celebes, if one does not prefer to have recourse for an explanation to immediate relations between India and America. Apart from a past connection between America and Asia, which is severed by the Behring Strait, there would come into consideration the past connection between western North America (Alaska) and the Sunda Islands still indicated by the chains of the Alentian and Kurile Islands, Japan, the Liu-Kin Islands, and the Philippines, and by the relatively moderate depth of the sea on the whole line" (I. pp. 235, 236).

(2) "The only geographical crime, with which Mr. Haase believes he must reproach me, is that I bring the Celebensian *rhesus* to American species, because its pattern points absolutely to these, so that I called it, with express regard to the contradiction in geographical distribution, a form that came accidentally, resp. was miscarried, from America to India" (II. p. 63).

(3) "The reproach of Mr. Rothschild against my *Artbildung und Verwandtschaft bei den Schmetterlingen* I., 'that I had apparently employed too small a material to enable me to avoid grave errors in respect to the relationship of the various *Papilios*,' is essentially founded on the circumstance—as far as I can make out from his paper—that I bring this Butterfly [*P. rhesus*] not to the Indian *anticrates-aristeus*, but, as a probably immigrated form, to the American *ajax*. What criticisms Mr. Rothschild has to offer in other directions relate only to differences in our opinion about the delimitation of species and similar matters, and I perfectly agree that even grave mistakes may innocently occur to somebody who is not in a position to have such collections at his disposal as Mr. Rothschild. I should, therefore, be the more grateful for the indication of an error, the greater the mistake were. But it must actually be proved, else there remains only **unjust reproach** [black type mine]. The same objections in respect to my opinion about *rhesus* I have already rejected in a reply to E. Haase. Against Mr. Rothschild I must remark that I bring *rhesus* to *ajax* not only on account of the number of the bands, which is six instead of seven, but also on account of numerous other characters in pattern (f. i., also connection of bands vii. and viii. behind [it should read viii. and ix.], also characters of the underside, ornamental band, etc.), also on account of

the outline of its wings. I must adhere to the position which I have attributed to *rhesus*, though their immigration to India must be assumed as **probable**."—The above remark about *Artbildung* I. was made by me, not by Mr. Rothschild: the "unjust reproach" lies, therefore, with me. Is the criticism unjust? In Nov. Zool. II. is said:—

(1) That *rhesus* has the same specialised venuration as *aristeus*, and differs in this from *ajax*:

(2) That the ♂ has the same cottony scent-organ in the abdominal fold as *aristeus*, thus being different from *ajax*;

(3) That the seventh band of the forewing is often indicated, sometimes well developed, such seven-banded individuals coming very near certain examples of *aristeus hermocrates*;

(4) That the pattern of the hindwing of *rhesus* agrees with that of *aristeus*; and

(5) That *rhesus* occupies a gap in the area inhabited by *aristeus* and its forms, namely Celebes.

Are these reasons really not convincing? To settle the question of *rhesus* once for all, I will add that *rhesus* agrees (as shown by me in Nov. Zool. III. 1896. pp. 488, 503) very closely with *aristeus* in the morphology of the end of the abdomen in both sexes, and disagrees entirely with *ajax*; that the antennae are the same as in *aristeus*, the joints not being subcarinate ventrally in the middle as in *ajax* and *philolaus*; and that the abdomen is white beneath and has indications of white rings in *rhesus* and *aristeus*, while *ajax* and *philolaus* have a black middle line for the underside of the abdomen and no white rings—distinguishing characters mentioned in *Artbildung* I. The above statement that *rhesus* was separated from *aristeus* and its forms also on account of "differences of the underside, ornamental band, etc.," is not intelligible, as the underside is in *Artbildung* I. expressly compared with that of *aristeus* (or a form of it), and not at all with that of *ajax* or *philolaus*. For we read in I. p. 219 of the underside of *rhesus*: "It is highly remarkable that a red spot stands separate in the external angle of the middle cell, similarly as in *aristeoides*, *aristeus*, etc., and further that in the following cell there is, just as in *aristeoides*, a black spot with a minute red one in front. . . . The transverse ornamental band stands in the same connection with the [longitudinal] ornamental band, and this connection is in the same way interrupted as in *aristeoides* and other members of the *leosthenes-antierates-ajax* group. The ornamental bands consist namely, as in *antierates* for instance, of two black-white-red-black-white-black band-sections. The anterior black part consists of one spot each, of which the inner one begins, again exactly as in members of the just-mentioned group (for instance, in *antierates*), to form a new ornament." Does this not mean that the pattern of the underside of the hindwing, especially the ornamental band, is nearly exactly as in *aristeus* resp. its forms? That the argument from the outline of the wings is invalid is shown (1) by *aristeus hermocrates*, the wings of which have **nearly** the same outline as those of *rhesus*, though the insect is smaller, and (2) by the well-known fact that a great number of Papilios (and Nymphalids) exhibit this same peculiar character in Celebes. On Einer's Plate IV. *philolaus* and *rhesus* look so much alike, and appear so different from other species, because they are both drawn with the wings in the same peculiar position.

The errors in the treatment of the Papilios we have been dealing with in the foregoing pages induced me to say of *Artbildung* I. that the classificatory results of

that work were of little consequence for the systematist. Eimer replies (*Orthogenesis* p. 47), that he is content with the fact that other workers have repeatedly expressed their open acknowledgment of his researches having opened quite new ways for classification. I am aware that in descriptive entomology the methods of comparative morphology are not generally employed, and it would certainly be a great success, if through *Artbildung* these methods became better known to a good many classifiers of Lepidoptera. It was not this I had in view when I wrote the above sentence. I meant, on the contrary, to state that the classificatory results in *Artbildung* I., i.e. the grouping, the kind of relationship which Eimer believed himself to have demonstrated as being correct, were of no consequence, simply because these results were to a large extent quite wrong. That other workers agree with Eimer, that may be; but I very much doubt that a single one of them has examined the facts upon which the conclusions are based. One may agree with Eimer in the belief that acquired characters are hereditary, and that Natural Selection is not **the** factor in Evolution, but disagree nevertheless with him in respect to the facts brought forward to "prove" those contentions. These general contentions are surely not new, their repetition will not help us, and the "proof" of their correctness is certainly not given by advancing observations which on closer examination are either fallacious or inconclusive. In *Orthogenesis* only one entomologist is mentioned as a supporter of Eimer's opinions, Dr. K. Escherich, the results of whose studies on the wing-pattern of a genus of Coleoptera, *Fonabris*, are quoted in *Artbildung*, i.e., p. 7. According to Escherich—I expressly state that I am not going to criticise that author, I merely mention his results here because they are said by Eimer to agree with his—there are four main types of wing-pattern in *Fonabris*, the wings being (1) longitudinally striped, or (2) spotted, or (3) transversely banded, or (4) unicolorous; the phyletically oldest pattern is the longitudinal stripes, which developed consecutively into spots, these into transverse bands, and resulted finally in monochromatism. [I mention for the sake of explanation that Escherich has adapted the wing-pattern of *Fonabris* to the scheme of development given by Eimer. Those four phases in the mutation of the wing-pattern form the starting-point of his research.] Escherich's **longitudinal** stripes are in the direction of the veins, and his **transverse** bands at right angles to them; Eimer's **longitudinal** bands are, on the contrary, at right angles to the veins (like Escherich's **transverse** bands), and his **transverse** bands correspond, morphologically, to Escherich's **longitudinal** ones. The result of Eimer's researches in Lepidoptera is that the bands across the veins are the phyletically older, while Escherich maintains for Coleoptera that the bands in the direction of the veins represent the ancestral pattern. Are the two results really in accordance with one another, as is maintained in *Orthogenesis* p. 7? The same kind of arguments which led Escherich to conclude that the steps in the development of the pattern were (1) bands with the veins, (2) spots, and (3) bands across the veins, induced Eimer to infer that the development had taken place in exactly the opposite direction.

What I have said will suffice, I hope, to enable the reader to come to an opinion about the correctness of the classification in *Artbildung*, and to judge for himself whether there was justification (1) for the assertion in *Artbildung* I. that the wing-pattern is the very best guide in tracing out the relationship of species of Lepidoptera, and (2) for my contention that the classificatory results were to a great extent wrong.

As we read in II. p. 59, "If my laws of the development of the pattern are correct, then my inferences as to the relationship based upon these laws must be right," one would be justified in accepting the inverse of this sentence, considering that those inferences are largely erroneous, namely: as the relationship deduced "with absolute necessity" from the "laws" of development of the pattern is not correct, the "laws" must be fallacious. But this conclusion would be hasty; for the most general "law," namely that the phyletic connection between allied forms can be demonstrated by a comparison of the organs of the forms, is certainly sound. This basis of comparative morphology will not be shaken, if an author who adopts it comes to erroneous results. That Eimer applied the methods of comparative morphology also to the wing-pattern can only be mentioned with praise; but that the application was carried out with a certain amount of looseness is shown by the strange results in the classification of the species, and becomes also obvious, if one examines the more general results which bear upon classification, of which the two principal ones are, (1) the deduction of the ancestral pattern of all Lepidoptera, and (2) the kind of development called Homoeogenesis.

The pattern of the wings of the ancestral Lepidopteron consisted, according to Eimer, of eleven "longitudinal" bands running over both wings at right angles to the veins. I will not enter into the question, whether Haase was right in maintaining these "longitudinal" bands should be called "transverse"; such a contest ends necessarily in a squabble about the proper meaning of ambiguous words. But it is self-evident that, if one calls a band in one group of Lepidoptera "longitudinal" if it runs across the veins, one cannot call it in another group "transverse" if it has the same position to the veins, provided that the veins in all Lepidoptera, nay, in all insects, are homologous. That the latter is the case cannot be doubted, and it is, therefore, a serious matter to maintain, as Eimer does (II. p. 49), that the bands of the forewing of *Noctuidae*—which run across the veins as in *Papilio*—might very well be called "transverse," because there are no corresponding bands, as continuations of the former, on the hindwing.

Eimer's contention in respect to the pattern of the ancestral Lepidopteron may be divided into two parts: (1) that the number of the bands on the wings of the ancestor of all Lepidoptera was **eleven**, and (2) that the bands were continuous, running from the costal margin of the forewing to the abdominal margin of the hindwing. The first point can be briefly disposed of. The only argument I can find in *Artbildung* and *Orthogenesis* for this part of the contention is, that all the different wing-patterns of Lepidoptera can be derived from eleven bands, namely the highest number of bands found in *Papilio podalirius*. Certainly, but their derivation from any other number of bands is just as easy to carry out, if one adopts Eimer's method. For he says (*Orthogenesis* p. 255) that the original eleven bands have been split up into more, if the number of bands is larger, and that bands have disappeared by fusion with others or by obliteration, if he finds a smaller number of bands. Eimer counts on the forewing in *P. podalirius* six bands from the base to the discocellular veinlets, and five between this point and the apex of the wing. In his figure of *Cethosia* (l.c. p. 117) there are from the base to the discocellular veinlets seven distinct black bands, an indistinct band, and a basal spot corresponding (according to Eimer's method) to one more band; these nine separate bands are counted by Eimer as five, while in other *Nymphalidae* which have less bands in the cell he counts every single band as one. In *Cethosia myrina* from Celebes there are eight bands in the cell; the *Brahmaeidae*, many *Geometridae* and other Moths

have a far greater number of well-marked bands, which have nothing to do with "Rieselzeichnung."

But of much greater importance than the number of bands is the question, whether the ancestral pattern did really consist, as maintained by Eimer, of continuous bands. If this point were demonstrated in Eimer's books by convincing evidence, if he had shown that the banded forms of a group of allied species were the phyletically older, the spotted and streaked forms the phyletically younger ones in all groups of Lepidoptera, nay, even only in Butterflies, this result would be worthy of the highest comment, and far outweigh all the mistakes in the special classification of the species.

I find five arguments brought forward in support of that contention, namely:—

(1) The streaked, spotted, and unicolorous wings are derivable from the banded wing.—Yes ; but exactly as the presence of spots is explainable by assuming that bands were broken up into spots, the presence of bands can be explained by assuming that spots had fused to bands ; and the same can be said of the development of spots from streaks, and of streaks from spots. The question is, have we to conclude that the line of development was from bands to spots to streaks, as Eimer maintains for Lepidoptera, or from streaks to spots to bands, as Escherich says of Beetles ? or was the spotted wing the original from which the banded wing developed in one, the streaked wing in another direction ? All three possibilities would equally well explain that there is a connection between the banded, spotted, streaked wings of different species.

(2) The series of allied forms put together in each group, says Eimer, demonstrate the road Evolution has taken in evolving one from the other, and give as strong evidence for the bands being the ancestral pattern, as the facts of Palaeontology furnish evidence for other conclusions in Evolution.—That the phyletic connection of the forms of Papilios as accepted by Eimer is to a large extent erroneous we have shown above ; but let us assume that in *Artbildung* the roads Evolution had taken were demonstrated, only for the sake of argument. If we thus know that there is a connection from one species to the other in a group of near relatives, the series of forms representing the road Evolution has taken, we have a road that leads both ways, from bands to spots and from spots to bands, and the proof of there being such a road does not provide us with the knowledge of the direction in which Evolution has traversed it, does not give an answer to the question, which steps in the mutation of the pattern are the younger, which the phyletically older ones, and hence there is no justification for a comparison with the facts of Palaeontology that do give an answer to that question.

(3) It has been shown in *Artbildung*, says Eimer, how minute characters appear in single individuals, increase in other examples, become more fixed, and appear as the characters of varieties and species, developing further in allied species, and thus form a connection between series of species, and such mutations have been demonstrated from the banded to the unicolorous wing, so that consequently the bands must represent the ancestral pattern.—I have not found an instance in *Artbildung* I. where it is shown that a banded wing develops into a spotted wing and then becomes unicolorous. The banded forewing of *Cosmodesmus* becomes unicolorous by obliteration and fusion of bands, and by a sudden change of the ground-colour into black, as demonstrated in *Artbildung* I. Where do the spots come in ? Further, the same series of species which demonstrate, according to *Artbildung*, the progressive development of certain characters A, demonstrate also, according to

Artbildung, the retrogressive development of other characters B, A beginning as minute individual characters and ending as specific and group characters, B beginning as characters common to a number of species, becoming in other species more and more obsolete, and ending as minute individual characters. Why is it A that demonstrates progressive development? Why not B? As A leads from the banded to the not-banded, and B from the not-banded to the banded wing, why must Evolution necessarily have taken the first direction? Because, says Eimer (*Orthogenesis*, p. 469),

(4) This cannot be: for "if the species which I consider to be the youngest were the phyletically oldest, my figurative tree would be reversed, the branches directed downwards"; that means that "numerous or almost countless forms would have developed all in the same direction towards a banded form; . . . we should have a polyphyletic tree."—This argument is of course quite invalid, even if the connection between the forms were really such as Eimer maintains. It is a contention of *Artbildung* that the Lepidoptera develop in the direction from banded to spotted wings: why could one not also contend that the Lepidoptera develop in the direction from spotted to banded wings? That has surely nothing to do with the question of the mono- or polyphyletic origin of Lepidoptera. The branches of the tree would be divergent, whether the ancestral pattern consisted of continuous bands, or of internervular spots, and in both cases there would also be convergent development in certain characters.

(5) But an unconfutable proof, continues Eimer, of the correctness of his opinion is given by the ontogenetic development of the wing-pattern in the wing of the chrysalis.—*Papilio podalirius* has, according to *Artbildung* I., preserved a pattern on the forewing which is similar to that of the ancestral form of the whole order. If this contention is correct, we must necessarily find that in the ontogeny of the wing-pattern of *podalirius* the first stages are still more ancestral than the pattern of the imago, that the markings appear as bands which then undergo changes leading to the special form of the imago bands; while, on the other hand, if the bands of *podalirius* represent younger phyletic stages, we must find that the first ontogenetic stages of the pattern do not consist of bands. Now, what is really found on the pupal wing of *podalirius*? The rudiments of the pattern of the forewing of *podalirius* in the pupa are, according to Haase and Countess Linden, internervular spots, which then fuse to bands. Ontogeny, therefore, does not prove what it is said in *Orthogenesis* to have proved.

The second general result of *Artbildung* which is of greater importance for classification is that in various forms (which do not stand in the connection of ancestor and descendant) a new character may appear which was not present in the common ancestor, and that we consequently meet with similar forms in not closely allied groups, forms the similarity of which is due not to immediate relationship, but to similarity in the direction of development, to **Homoeogenesis**. **I fully acknowledge that it is a great merit of *Artbildung* to bring to mind again and again that similarity is not always a sure sign of relationship. But if one recognises the bearing of this result on classification, one should be doubly careful in accepting similarity in one organ, in the pattern of the wing, as evidence of relationship, without further inquiry whether the assumed relationship is borne out by other organs. Homoeogenesis shows distinctly that a classification built up on one character or on a set of correlatively mutating characters has no sound basis. I leave it to the reader to consider whether there was *a priori* any great probability that the researches**

relating to wing-pattern only could fulfil what Eimer claims for them, namely that "by my researches the principal traits of the true relationship of the forms are ascertained."

We will now leave the classificatory results in *Artbildung*, and devote some lines to a review of a few of the conclusions relating to the origin of species. The great persistency with which Eimer has advocated that acquired characters are hereditary, that Natural Selection is of little importance in the evolution of species, is admirable, and it should be acknowledged with emphasis that he insisted from the first to the last on variation being definite. It was Eimer's opinion that he had accomplished the thorough defeat of Neo-Darwinism by showing (1) that mutation proceeds only in a few definite directions, (2) that these directions depend upon the constitution of the animal and the direct influence of external conditions, not on Natural Selection, and (3) that experiments with heat and cold have proved the direct mutating influence of external conditions.

Whether the directions of development are in my opinion few or many, I will not say; but it strikes me that, according to *Artbildung*, every *a priori* possible direction of the development of the pattern occurs among the Butterflies; for we learn from *Artbildung* and *Orthogenesis* that new forms may originate (1) by the appearance of new characters and by the modification of old ones, and that the modification may take place (2) in a postero-anterior or antero-posterior direction, (3) in an infero-superior or in a supero-inferior direction, (4) on the fore- or on the hindwing, above or below, (5) in a basi-apical or in an apici-basal direction, (6) progressively or retrogressively, (7) gradually or *per saltum*, (8) in one character of a species in one direction, in another character in an opposite direction, and so on. That Eimer has not always been successful in ascertaining whether the facts bear out conclusions as to the "laws" of the direction of development in the evolution of the pattern, and as to the causes that govern the direction of development, may be seen from a few examples. The bands of *P. podalirius* are said to be inclined to disappear first on the upperside, which is in accordance with the statement (I. p. 115) that in the *podalirius* group the underside shows everywhere the more original condition. The only band of the forewing that is liable to disappearance in *P. podalirius* is band vii., a band that is very often mentioned and its variation described in the chapter on *podalirius* in *Artbildung* I.; but just this band, if not obliterated, is either **present** on the upperside and **absent** from the underside, or is at least larger above than below. As band vii. is a band of the original pattern according to *Artbildung* I., the **upperside**, not the **underside**, shows here the more original condition of the pattern.

We are told in *Artbildung* II. that the spring form of the Central European *P. machaon* has the phyletically older pattern, the summer brood, the Mediterranean and Asiatic forms, the younger pattern, and it is also stated that *xuthus* originated from *machaon* and "stands in connection with the **still more modified xuthulus**." Now, if in *machaon* the summer brood is the more advanced, how then can it be explained that in the species said in *Artbildung* to be derived from *machaon* the **winter brood (xuthulus)** is **more advanced than the summer brood (xuthus)**? The evidence brought forward for the contention, that the line of development of the pattern was as here maintained, will not be convincing to anybody. The reader will remember that I said before that Eimer did not know that *xuthus* and *xuthulus* stood in the relation of summer and winter form.

In *Orthogenesis*, p. 471 (note), a reference is given to Doherty's observation that there is a dry and a wet season form of Butterflies in India, this observation being advanced as an argument for the contention that the leaf-like form of certain Butterfly-wings is immediately due to the direct influence of external conditions, **such as heat and cold**, not to selection. The reference reads: "According to Doherty and De Nicéville, moisture and dryness (dry heat!) have great influence upon the shape of the wing." No, it is not **dry heat** which produces the dry season form in Northern India; the dry season is the cold season!

If we notice that, according to *Artbildung*, a character develops in exactly the opposite direction in members of one and the same species, one should conclude that this phenomenon was due to differences in the local conditions of life. Though this is conceded in many places in *Artbildung*, yet the author was so convinced of a difference in the constitution of the insects being really the *prima causa* of the direction Evolution takes, that he contends that insular forms are not necessarily the outcome of the special conditions of life of the locality, but may originate because the inert general directions of development remain entirely potent in the new locality (II. pp. 9, 10). "Much more important changes in the original direction of the development," continues the author in *Artbildung* II. p. 11, "than result from external conditions in connection with local separation, occur frequently in the middle of the area of a species, and lead either gradually or suddenly to the origin of new species. . . . By these facts, for which the Swallow-tails furnish specially prominent examples, the importance of geographical separation for the origin of species is much diminished." What are called "facts" here are contentions. Contentions are not facts before they have been proved. Let us then see the evidence upon which this proof is founded.

(1) "*Abarten* may be geographically separated or not. For '*Abarten*' originate also in the midst of the individuals of the parent form, as is self-evident from the laws of definite direction of development, or Orthogenesis. Such '*Abarten*' become gradually . . . species."—Is it really self-evident? No, these "*Abarten*" will not become "*Arten*," though the species may become dimorphic.

(2) "*Papilio protesilaus telesilaus* occurs in the midst of the area of *P. protesilaus*, where it has perhaps also originated" (II. p. 10).—Whether *telesilaus* has originated in the midst of *protesilaus* is the question at issue, which must not be merely assumed to be answered.

(3) "The *asterias* group has originated in the midst of the area of *machaon*" (II. p. 11).—Do we know that? If *asterias* is a derivation from *machaon*, what facts are against its having originated as a geographical race? *Machaon* could have subsequently migrated into the area of *asterias*, and the latter into that of the former.

(4) A case similar to that of *asterias* (II. p. 11) we meet with in the *turnus* group, where, as "*Abart*" of the *female* of *P. turnus*, the blackish "*Abart*" *glaucus*, which is also in other respects somewhat modified as compared with *turnus*, suddenly appears.—This illustration of the origin of species in the midst of the area of the parent form is not well chosen, because *glaucus* is not in the midst of *turnus*; it is a south-eastern *female*, that occasionally is found farther north. It is also not correct to say that it has suddenly originated; there are transitions to the ordinary form known, and there is no evidence against this black form having been evolved gradually, instead of *per saltum* as maintained in *Artbildung*.

(5) "Epistasis it is (*Orthogenesis*, p. 21) by which new species may originate

everywhere without geographical separation. For, if a greater number of individuals proceed in a certain direction of development, while others remain behind, a new species must necessarily spring up. This progression of a greater number of individuals can take place in the middle of the area of a species, if these individuals are more sensitive against the external conditions than the remainder of the species.—Is the greater individual sensitiveness hereditary in all the offspring of the more sensitive specimens? This is what we have to prove; we must not merely assume it. For, if the new and old form, resp. the offspring of the “sensitive” specimens and the less sensitive ones, mix, the parent stock will **not** remain stagnant, as “Epistasis” implies; it will follow the more sensitive individuals. And as there will be differences in the degree of sensitiveness in the parent stock as well as in the assumed new form, it is not intelligible how a gap that would separate the one original species into two can come about.

Orthogenesis may be a process in Evolution, but it is certainly not a cause. The question is, which of the many possible general lines of development will be followed by the geographically separated members of a species, and there is nothing in the above arguments which shows that the eventual course to be followed by a species in a certain area does **not** depend on the biological conditions of this locality. An individual has many characters, a race many individuals, capable of varying in different directions. A general force, gravitation, brings the particles of the water of a river onwards; the direction of the movement of every molecule is—as every “direction”—geometrically straight at every place, at every moment, but the meandering course of the river depends not on that general force, but on the external conditions the water has to cope with.

At the bottom of the conclusion that species originate in the way as maintained in *Artbildung* is the opinion to which expression is given in I. p. 16: “It is a main object of my researches to prove, that the same factors which are the cause of the aberrational characters of individuals, and produce the ‘Abarten,’ must also give rise to species: this follows irrefutably already from the fact that the distinguishing characters of species are the same as those of ‘Abarten,’ and the characters of the latter the same as those of individuals.”—The reader who is not more closely acquainted with the insects upon which the researches in *Artbildung* are based, may easily be deceived by the arguments in favour of the above contention—a contention it is, not a “fact.” For he is liable to overlook (1) that in *Artbildung* **aberrant individuals** are treated either as aberration or as “Abart,” just as it is thought best in that place, (2) that forms of dimorphic species are designated as “Abarten,” (3) that different broods of the same country are considered “Abarten” and “Abartungen,” (4) that one and the same individual aberration, or seasonal form, or dimorphic form are treated in the descriptive part of *Artbildung* correctly as what they are, while in the general part, where the conclusions are drawn, they appear as “Abarten,” or even “Arten.” Thus we learn in II. p. 23, that the black female of *P. turnus* is an “Abart” (*P. turnus glaucus*), while on p. 28 we find the same form designated as a species (*P. glaucus*), and on p. 142 as *P. turnus* var. *glaucus*. The individuals of *Papilio podalirius* with 11, 10, 9 bands are correctly said in I. p. 41 to be individual aberrations, unfortunately named *P. podalirius undecimlineatus*, *P. p. decemlineatus*, *P. p. novemlineatus*, while in *Orthogenesis*, p. 48, that same aberration *undecimlineatus* is brought forward as an “Abart” of *podalirius*. Dark individuals of *P. philolaus* are described as aberration and named *P. philolaus nigrescens*; melanistic specimens are said in *Orthogenesis*, p. 49, to form an aberration of the aberration *nigrescens*, and

are called *P. philolaus niger*. The individuals of *P. machaon* with two black dots on the forewing above between veins 6 and 8 are correctly treated in II. p. 26 as aberration: p. 26 as "Abart," *P. machaon bimaculatus*. The North African spring brood of *P. podalirius* is called "Abart" *P. podalirius feisthameli*, the summer brood "Abart" *P. podalirius latteri*, etc. By thus calling the same form here "Abart" and there "aberration," or even "species," and by using the same terminology for aberrations, seasonal forms, and geographical races (*P. podalirius undecimlineatus*, *P. podalirius latteri*, *P. podalirius virgatus*; *P. machaon bimaculatus*, *P. machaon asiaticus* [= *sikkimensis*]), it is certainly **not** proved that aberrational and subspecific characters are the same. Geographically separate races are entirely different from aberrations, seasonal forms, and forms of dimorphic species that occur in the same locality. A comparison of the variation of different organs, for instance of wing-patterns and copulatory organs, reveals that at once. The combination of distinguishing characters of aberrations and seasonal forms is different from the combination of distinguishing characters in geographical races as shown in Nov. Zool. III. 1896. pp. 499—501. And this diversity in the combination of the characters that constitute an aberration, or a seasonal form, from the combination of characters that constitute a geographical race, shows clearly that correlation—so often advanced in *Artbildung* as an important factor in the ramification of species—has little to do with the origin of geographical races. That the latter are of the highest importance in the divarication of species, that they are the true **subspecies**, forms in the process of being evolved into new species, is proved by the great difference in the physiology of the two kinds of varieties. For the offspring of an intercrossing between well-marked aberrations of a species are not intermediate in characters between the parents, but belong either to the one or to the other aberrational form, while the offspring produced by an intercrossing between geographical races are, as in the case of an intercrossing between different species, intermediate between the two parent races.

We know that individual aberrations are often confined to a certain portion of the area of the respective species, that in other cases the aberrational characters appear regularly in a greater number of individuals of a locality, and that in others again all the individuals of that district possess certain distinguishing characters (compare Nov. Zool. III. 1896. p. 477). The development of geographical individual aberrations leads to geographical races; the development of non-geographical aberrations leads to dimorphism. Now, as the combination of physiological—such as relate to propagation—and morphological characters in marked non-geographical forms is different from that in marked geographical forms, we must conclude that the two forms are different in kind in so far as factors come into play in the evolution of geographical races which do not act in the case of non-geographical forms of a species, and that we have, therefore, to distinguish between causes of "aberrational" characters and causes of "subspecific" characters. And as the combination of physiological—as shown by crossing—and morphological characters is in geographical races the same as, but to a lower degree than, in species, it is obvious that the causes upon which depend the evolution of subspecies, = geographical races, are the same which lead to the origin of new species.

In conclusion of this review, which I am sorry to say is mostly destructive, I will not omit to point out that Eimer's researches on Lepidoptera, though full of errors *re* facts and loose in argumentation, are nevertheless of great interest for the classifier as well as the general biologist. For the very boldness in language with

which the problems are attacked, the numerous contentions in *Arthildung* and *Orthogenesis*, the constant repetition that this or that contention is proved to be correct, will serve to bring the study of Lepidoptera, to which Eimer has drawn attention, onwards by instigating others to verify the facts and examine the arguments. For this Lepidopterists can only be thankful.

ON THE BIRDS OF LOMBLÉN, PANTAR, AND ALOR.

By ERNST HARTERT.

PRACTICALLY nothing has hitherto been known of the ornithology of these islands, lying in a line from Flores to Wetter, although Doherty had collected butterflies in all of them, but no birds. Everett's exploration of these islands is, therefore, of great importance. Altogether the birds prove that the Flores ornithology reaches to Alor with but little alteration, while the ornithology of Wetter has already a greater proportion of modified forms. The ornithology of Lomblén, Pantar, and Alor (or Ombay) is chiefly the same, but in some cases that of Alor differs, and probably has received some Timorese immigrants, while Lomblén and Pantar are more purely Floresian. These facts would probably be more striking if the collections from Lomblén and Pantar were larger.

In Alor Everett collected chiefly in the eastern end of the island (Irána), where there was a small river, but he was not satisfied there. He then went by boat to Lantutuka, in Flores, stopping four days at Lomblén *en route*, but finding the mountains everywhere inaccessible, owing to there being not a drop of water on them. At Mount Wokka he found fighting going on; in fact all these islands, except Alor and Pantar, were just then in a state of absolute anarchy. During the Alor trip Everett and his men frequently could not get enough to eat, and the water was always bad, and they had a good deal of exposure in open boats under a terrific sun. "The result was," Everett writes, "that I got a severe attack of intermittent fever, and when off my head I think I must have kicked violently against something with my damaged leg—anyhow I burst a vein and the leg swelled to an enormous size. It was kept bandaged with ice for a week, and ultimately I was taken to the hospital in Makassar, where I am now slowly recovering from an operation. The Alor collection of birds cannot be regarded as at all an exhaustive one. It is sufficient, however, to show that the Flores ornithology reaches its limit there. I could find no trace of an *Eclipticus* in Flores, or any of the islands up to Alor. *Trichoglossus* and *Geoffroyus* were not seen, and the natives did not know them. There is a *Geocichla* in Alor, but I failed to get it, and my hunters twice saw a bird which they identified with the *Seythrops*. An *Elanus* was once observed. A single *Gerygone* was shot, but too damaged for preservation. *Gallus furcatus* is common. Other birds identified beyond doubt in Alor, but not sent, were *Pandion leucocephalus*, *Haliaeetus leucogaster*, and *Tringoides hypoleucus*. My principal object in visiting Alor was not attained, viz. the ascent of the mountain at the eastern end (6000 feet!), and it can only be achieved during or immediately after the rainy season. I would have made a longer stay in Lomblén, but I had rice only just enough to carry my party to Lantutuka. Neither I nor my men could subsist on maize, which